CHANGE OF TYPE IN DISEASE.

THE

ADDRESS IN MEDICINE

DELIVERED AT THE

THIRTY-THIRD ANNUAL MEETING

OF THE

BRITISH MEDICAL ASSOCIATION,

Held in Leamington, August 1865.

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[*Reprinted from the* BRITISH MEDICAL JOURNAL *August 12th* 1865.]

LONDON:

PRINTED BY

T. RICHARDS, 37, GREAT QUEEN STREET.

MDCCCLXV.

Notes on Medicine

CHANGE OF TYPE IN DISEASE.

GENTLEMEN,—Before entering on the subject of the Annual Address, it is right that I should offer to the Association my thanks for the honour done to me in the request that I should deliver the Address in Medicine in 1865. It is unbecoming for any one to speak of his own labours; but, if I have a claim to your consideration, it is that, for the whole of my professional life, I have sought to improve the social position of medicine in this country. How far those efforts have been successful, is not the question here; but that this has been my object, I may simply aver. And here let me declare that which I believe to be true, that the cause of medicine, taken in its broadest sense, whether as to its social, political, or scientific relations, is to be advanced more by the cultivation of the minds, the morals, and the manners, of those who are engaged in it, than by all other influences whatsoever.

But, in your selection of a member from Ireland, I feel that you have honoured its School of Medicine—a school of which all portions of the United Kingdom may justly be proud—a school of which the leading feature has been its devotion to practical medicine, surgery, and midwifery; that is to say, that the application of every discovery in physiology, chemistry, and pathology, to the purposes and ends of the healing art in its widest signification, is, and has always been, the great object of our teachers.

There is a fitness, at all events, in a member of that school appearing before an Association mainly composed of the workers in medicine, who bear the burden and heat of the day, and the chill, and

darkness, and storm of the night, bringing, as they best may, health and counsel and comfort to the suffering man.

If we look at the contributions to medicine by the Irish School for the last half century, we shall find that, with a few exceptions, they consist of works, reports, memoirs on clinical medicine, surgery, and midwifery, or of researches in pathological anatomy, mainly having reference to the diagnosis of disease. As compared with other schools, we have not much to show in the way of discovery in pure anatomy, in animal or vegetable physiology, in microscopic anatomy or pathology, or in organic chemistry; but what the School has done, or attempted to do, is, on the one hand, to enlarge our knowledge of medicine and surgery by careful clinical study of the characters and history of disease; and, on the other, guided by a sound eclecticism, which neither rejects nor blindly adopts a newly announced principle or observation, to test it by the light of experience, and find how far we can give it a place among the aids, the practical aids, of the healing art.

This character or tendency of the Irish School may be traced to traditional and accidental causes. Among the latter, one may be the existence of so large a number of hospitals in Dublin, which, with a population of about 300,000, has not fewer than twenty-one hospitals, each with its distinct staff, the members of which are appointed for life. In this way, the attention of a large number of the young men of the profession was, as they became hospital officers, necessarily turned to clinical study.

Medicine, in its great quality as a practical art, advances in many directions; of which two may be indicated as the most important.

One is the discovery of new facts, whether relating to physiology, pathology, or therapeutics, each of which, even although its practical bearing be not apparent, enlarges the boundaries of the field of certainty.

The second is the application of those new facts, on the one hand, to testing the value of methods

long in use; and, on the other, as a guide in exploring the wilderness of the unknown which stretches around us, which we are seeking to discover, and which we hope in time to reclaim.

For example, it has been long admitted that internal solutions of continuity are so often attended with new, unforeseen, extraordinary, and, above all, sudden symptoms, that their occurrence may be taken as characteristic of this class of internal lesions, as applied to the viscera. Laying hold of this fact, we inquire, Does this formula apply to solutions of continuity of the fluids—say blood? Now, it is found, that new, sudden, and extraordinary symptoms referred to a particular organ, may often so occur, and yet be without change perceptible to any mode of investigation. But the researches of Virchow show that, even here, the truth of the principle is established; for there is, in one sense, a solution of continuity, not indeed of the tissues of the suffering organ, but in the current of the fluids which supply it. An embolus suddenly obstructs an artery, and causes symptoms having a common character, though, of course, varying according to the organ affected—symptoms which are new, sudden, extraordinary, often violent. Here the embolic pathology confirms principles already in use so far as the solids are concerned, and extends their application to the fluids.

Take it, again, as a lamp for guiding us to new knowledge, and therefore new power. The embolic pathology has at once discovered, as it were, a new set of diseases. It was long known, that interruption of the arterial supply would induce the death of parts for which that supply was intended, as in the disease described by Mr. Pott. Here the deficiency is caused either by feebleness at the centre of circution, or disease at the extremity of the arterial tree. The process, as might be expected, was slow and gradual. But, in this newly discovered class of diseases, we observe the sudden obstruction of a nutritive artery from an embolus, which often proceeds from a disease of one of the valves of the heart; so that,

from the sudden occurrence of paralysis of function, we may be led to suspect the existence of an embolus, and to seek for its source. But what if we could see the embolus in the central artery of the retina, as Liebreich did, in a case of sudden and complete amaurosis, and so was led to the diagnosis of a disease of the heart never before suspected to exist?

Here the new observation or discovery exhibits its twofold value. It confirms an important principle already acted on—it leads to a new mode of discovering changes far remote in nature and in seat from that which is the immediate subject of inquiry.

If we look at the collective mass of our brethren over the world, we may distinguish two groups, with lines of demarcation not indeed very sharply defined, but yet sufficiently distinct to justify their separation into categories. In one we find the workers in experimental physiology, in pathology, and in animal chemistry. I use the latter term, because it is not yet established that the laws of the inorganic chemistry are identical with those which operate in the living body. In the second category, we place those who practise the healing art as their daily calling, among whom is to be found a large mass of thinking men, who fulfil the great function of testers of the value of newly announced discovery—men whose minds, originally strong, and essentially of an eclectic mould, are trained and shaped by working on a more extended field, and by having to employ a more difficult, because less mechanical method.

Now, it is in this class that we find that great body of observers, among which medicine can point out most of her representative men; and this is true for all time past, and for the present also. The observers of disease in the living man, and the faithful recorders of its phenomena, these are or have heretofore been the men who have made medicine a science worthy of respect, even before the introduction of physical means of diagnosis; even further, before anatomy was known—before physiology had shed its light upon life in health or in disease—before chemistry was a science—on to the time of Morgagni and

Bonetus, who studied the changes of organs in disease,—before the microscope, before the employment of those beautiful instruments and methods which in our day have done such wonders in advancing the certainty of our art—the ophthalmoscope, the laryngoscope, the endoscope, the galvanometer, and spectral analysis.

But all men are not, and cannot be, both observers and recorders. In the highest and in the lowest classes of our profession, the lets and hindrances of the daily work are so many and so great as to prevent the mass of practitioners from adding the fruits of their observations to the written records of medicine: whether it be from want of opportunity, of ambition, or of time or taste for writing, they simply work, apparently contented with the knowledge provided for them. But even in the hands of such the science does not stand still; for in their individual cases, at all events, it roots, and grows, and flowers; and so, it may be unconsciously to themselves, they acquire more and more the power of dealing with disease. For observation, even though it be best rendered fruitful by study, has its silent influence on our afterthoughts and actions, even when the special fact or circumstance is forgotten. So it happens that in this class there are many who advance the cause of medicine, inasmuch as they are in themselves exponents of its advance, and must influence more or less all with whom they come in contact.

We are now to study that state of mind by the help of which medicine is made useful. To acquire and improve this *mens medica*, as it has been termed, should be the labour of our lives. To find out its nature will not be a misspending of our time.

You will easily anticipate me, when I say that the condition of mind implied by the term in question is that which makes the good physician. It is not the age of the world that produces such a result, for there have been great physicians and surgeons from the earliest historic times. It is not—to put this in other words—it is not the number of established facts

in medicine, nor the amount of such facts known to the particular individual, but it is the power of rightly using those which he does know.

And hence we can at once perceive, that what makes the great physician is less the possession of knowledge of isolated facts, no matter how numerous these may be, than that greater quality of judgment based upon observation—a function of the mind, which, like many others, may be indefinitely cultivated and developed, so that in its exercise it may become (if I am permitted to use the expression) an acquired instinct. This power of balance and of combination—ready for use at all times and in all emergencies, exercised rapidly, almost unconsciously, and leading its possessor to do that which is best and safest under the circumstances—is that which stamps, not only the great physician and surgeon, but all those who are leaders in government, in arms, in art, or in the liberal professions. We can conceive a practitioner at the present day who knows all the ascertained facts in physiology and pathology, and who may be, notwithstanding, inferior to many who have lived more than a thousand years ago. There is no more decided evidence of an unexpanded mind in our profession, than the decrying the knowledge and usefulness of our predecessors. This was the fault of Paracelsus and Broussais, and in the present day we do not want examples of it.

Looking at the state of medicine in our day, and putting aside the consideration of its vast advances in power and usefulness, as derived from discoveries in physiology, pathology, and diagnosis, we observe that there are some great questions still waiting their solution—questions combining considerations so wide, that they may be said to apply to every branch of the healing art. I will indicate one of such questions—namely, that of the change of type; first, as regards essential diseases; next, as to local affections.

There are many of us who can remember the treatment of fevers and of acute diseases in our student days, characterised by a free use of general and local bleeding, and the employment of other decided

antiphlogistic methods. Such practice has now fallen, at least in these countries, into disuse; and even on the continent the employment of an opposite method has been gaining ground.

We can hardly conceive a revolution in practice more complete. Venesection is now, from being the most frequent, the rarest of operations. In place of the loss of blood, we have the exhibition of stimulants; in place of a system of almost starvation, we have the careful use of nutriment.

This change has given rise to the charge against our predecessors and teachers, that they were bad practitioners, ignorant of true pathology, little better than blind followers of traditional error. Not only has their power of observation been questioned, but their morality and honour have been assailed; for it has been suggested that the doctrine of change of type was an invention to cloak their former errors.

It is interesting to note that this is not the first time that charges of the same kind have been brought against the profession. Of these, the most remarkable was that of Broussais, who arraigned all existing and former practitioners for not treating fevers and acute diseases by local bleeding and starvation. Can there be stronger evidence than this, that our modern practice is not a novelty? All his predecessors were in error, because they practised as we do now. I say that this charge was remarkable, inasmuch as its author's views largely influenced European practice for many years.

But the thinking man finds it hard to believe that the fathers of British medicine were always in error, and that they were bad observers and mistaken practitioners. They, indeed, have rested from their labours, but their works remain; and he who reads the writings of Sydenham, of Haygarth and Fothergill, of Heberden and Fordyce, of Gregory, Cullen, Alison, Cheyne, or Graves, must have a very inapprehensive mind, if he fail to discover that there were giants in those days, and that the advocacy of such ideas only indicates a state of mind not consonant with the modesty of science.

The declaration that it has been or can be proved by a more advanced pathology, that bleeding never was the proper remedy for fevers and inflammations, has as yet no scientific ground. It is not yet given to us, notwithstanding all our advance in normal and in morbid anatomy, in the physiology of health or in that of disease, to be able to say, from the most minute examination of the dead organ or structure, what were *all* the conditions which attended it during life, in health or in disease—what were its local vital phenomena, what was its accompanying constitutional state. The words of Goethe, so well rendered by Dr. Anster, convey a deep practical lesson to those who would base medicine on anatomical change:

> "Alas! the spirit is withdrawn—
> That which informed the mass is gone.
> We scrutinise it when it ceases to be itself,
> Finger and feel it, and call this
> Experiment analysis."

But let us ask, Which is the most probable of these two suppositions? First, that our predecessors, including such as I have named, were bad observers, incapable of divining the truth, and blind adopters of an antiquated and mischievous method; or, secondly, that the type of disease has changed, and that in our own time. It happens, fortunately, that we can examine two living witnesses of great authority in this matter, and can refer to the works of two more who have left us their written testimony. Dr. Watson and Dr. Christison are still among us, in health and intellectual vigour—long may they be so; Dr. Alison and Dr. Graves have been but lately removed.

Now, all these testify that the character of diseases has in our time changed from a sthenic to an asthenic type; that is to say, from a condition in which inflammatory reaction was the prominent feature, to another where that state was absent, or, if present, only ephemeral—a condition observable in essential and in local disease, in which the antiphlogistic treatment was well borne, and productive of great relief,

to one in which a tonic and stimulant and supporting system was found the best method of guiding the disease to a happy termination.

It is very important to note that these views were not formed from any historical study of the recorded labours of others, but come before us as the actual observations of the great men whose names I have stated to you. They tell us that which they know—that which they themselves have seen. If we refuse this collective though separate and independent evidence—if we hold, with Professor Bennett and with Dr. Markham, that the doctrine of change of type is untenable—we must believe one of two things, either that these distinguished men were themselves deceived, or themselves deceivers. From this alternative there is no escape.

Let us hear Dr. Alison:—"When we reflect on these facts, we cannot think it unlikely, that the result of the inquiry which I have stated as so important, may be to show either that all causes capable of exciting diseased action in the animal economy, or, more probably, that the liability to diseased actions in the different departments of the animal economy itself, are subject to variations, which are made known to us only by the variation of such phenomena themselves; occurring merely in the natural course of *time*—an element affecting all vital phenomena quite differently from its agency on inanimate nature; and the effects of which, on living beings, we must take as ultimate facts, to be carefully observed, arranged, and classified, but which we are not to expect to be resolved into any others, which the study of this department of the works of Providence presents."

When I read these words of Alison—the best man I ever knew—it is with a feeling of wonder how it has happened that men should forget what reverence is due to his memory; whether we look on him personally as a man of science and a teacher, or at his life as an exemplar of that of a soldier of Christ. It was my good fortune to be very closely connected with him during my stu-

dent days in Edinburgh, and to attend him by day, and more often far into the night, in his visits of mercy to the sick poor of that city, to whom he was for many a year the physician, counsellor, and support. This was forty years ago, and at that time he recognised the change. Often has he said to me, "We cannot bleed this man; we must get him wine"; and the wine was got, and given with an open hand, so long as it was required. He used to say, "I am not anxious to put these poor people into hospital; they will get on better at home, if we are guided by looking at their constitutional even more than their local state." This, however, has been well put by Dr. Watson, who dates the commencement of the change from that of the first presence of cholera in London in 1833. We can easily believe, however, that the change in question would not occur in all parts of these countries at the same time.

It is very important, however, to connect that which I have now detailed to you with the observations of Alison, published at the request of Dr. Christison in part in 1850, and afterwards in 1856, in another memoir, entitled *Reflections on the Results of Experience as to the Symptoms of Pulmonary Inflammation, and the Results of Blood-letting, during the last Forty Years.*

In 1856, appeared Dr. Christison's *Memoir on the Changes which have taken place in the Constitution of Fevers and Acute Inflammation, in Edinburgh, during the last Forty-six Years.* This is a memoir eminently characteristic of its author; full of views and arguments which it becomes much more convenient to ignore than easy to confute. Dr. Christison shows, that the change of treatment in acute diseases is to be considered with reference to fever, as well as to local affections. He bears witness that the abandonment of bleeding in idiopathic fevers preceded by a good many years its abandonment in acute inflammation; and that this change in practice took place gradually, in all acute inflammations, not alone in pneumonia, because of the improved diagnosis of the disease, but in all others, in many of

which no sensible progress in diagnosis had been made. Looking at the epidemics of fever in Edinburgh from the beginning of the present century, he shows conclusively that, in 1817-20, and in 1826-29, their characters were those of Cullen's synocha and synochus — inflammatory, relapsing, critical. Speaking of the epidemics of 1817-20, he dwells on the hard, incompressible pulse, the ardent heat of the skin, the florid hue of the venous blood, and the impetus with which it escaped almost *per saltum* from the vein, the vivid glow of the surface, and the distracting pain and pulsation of the heart and chest. Similar phenomena occurred in the epidemic of 1826-29; and, in both, bleeding was largely practised with the happiest effects; so that, in the epidemic of 1817-20, the mortality, which was at first one in twenty-two, fell to one in thirty—a result which disposes of the charge of malpractice against the profession. But, in 1834, Dr. Christison found that probably for two years previously a change had been going on:—synocha had disappeared; synochus had lost vehement reaction of its early stages; typical typhus was much more common; and what did not come up to Cullen's mark of fully formed typhus was what physicians would now commonly call mild typhus, with more of introductory reaction than we observe now, but with less than in the two epidemics of 1817-20, and 1826-29.

"Accordingly" (says Dr. Christison), "I doubted, and all the physicians of our hospital also doubted, whether blood-letting was applicable as a remedy to that fever. We could not bring about resolution by a sweating crisis with it; we could not lessen by it the depth of the typhoid prostration; and, worse than all this, our patients ceased to sustain free venesection, a few ounces of blood bringing on faintness, and the constitution refusing to rally afterwards."

Lastly, to prove that this statement is not the result of an afterthought of the present day, Dr. Christison refers to his clinical lectures, delivered between 1833 and 1835, to show that he then declared the necessity of a change of practice.

I shall not apologise for giving you another quotation. Speaking of the theory or generalisation of the facts ascertained, he says:

"In epidemic fevers, a change may take place in the constitutional part of the fever; and this change has been exemplified in Edinburgh during the last forty years, by a transition from the sthenic or phlogistic character in the first twelve years to the asthenic or adynamic character in the twelve years which have just elapsed."

And he adds these remarkable words:

"If this change be admitted to have been proved, there is an end to all difficulty in accounting for the abandonment of blood-letting in the treatment of our fevers. In point of fact, I am able to state very positively, that the abandonment of bleeding in fever was suggested by the observation of a change in the constitution of fever, and in the effects of the remedy on it, and not by any other circumstance, whether extraneous or intrinsic. It is impossible to ascribe such change of practice, as Dr. Bennett has done in the instance of pneumonia, to an improved knowledge of disease. We have improved our knowledge of fever so far as to have been, for some time, well acquainted with the form of enteric typhus (dothinenteritis), which was unknown, or not recognised, at the commencement of our epidemics. But this is a rare form of fever in Edinburgh, scarcely belonging to its epidemics at all. And as to our only undoubted epidemic fevers, typhus and synocha, with their intermediates, we cannot be truthfully said to be better acquainted with them in 1857 than we were in 1830.

"I have given, I hope, a sounder explanation; less flattering, perhaps, to the rising generation of physicians, but surely more honourable to physic itself, more creditable to medical observation and experience, more consonant with the advanced state of medical philosophy. My own convictions on the subject are so strong, that I regard nothing as more likely, than that in the course of time some now present will see the day when a reflux in the consti-

tution of fever will present it again in its sthenic dress, and again make the lancet its remedy. And in that event it is not impossible that, while we are now charged with giving up blood-letting, because it was discovered to have never been the proper method of cure, we shall hereafter be assailed by some new enthusiast in blood-letting, who, in imitation of Dr. Welsh, and regardless of the fate of his doctrines, will accuse us, with equal justice, of having made our late fevers asthenic and typhous by blindly withholding their fittest remedy."

In truth, the alternation of the epidemic character of sthenia and asthenia is established in the case of eruptive fevers, and by an observer who has been held up as a ruthless spiller of blood—Professor Gregory. He has described an epidemic of measles which occurred in 1807 and 1808 in Edinburgh, and which, he expressly states, was not to be treated on the antiphlogistic plan, but rather by tonics and stimulants. This fever was preceded by the inflammatory measles and scarlatina, in which the lancet was used with advantage until the type again changed, and the asthenic fevers, as we have had them for a quarter of a century, reappeared.

The change of type, too, of the local acute inflammations followed, as might be expected, that of the essential diseases; and the change in treatment resulted, not from any new light shed on the practice of medicine, not from any new views in pathology, not from our advance in diagnosis, vital or physical, but from the observation of the general symptoms on the one hand, and the results of treatment on the other.

I may now add the results of my own experience in this matter. I remember the period when the change of type took place in Ireland; and am under the impression that it was observed earlier in Ireland than in Scotland, or at least in England. The great epidemic of fever in 1828 was a remarkable one from its compound nature, and seemed to be made up of synocha, synochus, and enteric typhus. But nothing was more remarkable than the vehemence of the in-

flammatory reaction in many cases; and it is a curious fact that this was sometimes seen at its highest pitch in the relapses, when it was often far more violent and dangerous than in the first attack. Local bleeding was largely employed. In many cases, venesection or arteriotomy had excellent results; so that, although there were abundance of cases with prostration, and others marked by the typhoid condition, the old sthenic character had not disappeared. The amount of wine used at that time in hospital was quite insignificant as compared with its consumption for the last twenty or twenty-five years. In Dublin, at least, this epidemic passed into one of intermitting fever; and it was then that I ventured on testing the nature of the practice recommended by Dr. Mackintosh of bleeding in the cold stage. The result of the experiment was against the use of the lancet; but I mention it, as indicating the time when it may be said that venesection was abandoned in our wards.

Thus, between 1822 and 1828, the sthenic character of essential and of local disease existed, and the lancet was freely used, often, as I believe, and as I have elsewhere stated, with too great freedom; but I well remember observing the frequent occurrence of the phenomena mentioned by Dr. Christison—the vehement action of the heart, the incompressibility of the pulse, the vivid redness of the venous blood, and the force with which it spouted, almost *per saltum*, from the orifice in the vein. I have myself taken as much as sixty ounces in a case of active congestion of the brain, with hemiplegia, before any impression was made on the arterial excitement: in this case, complete success followed. In rheumatic fever, too, we found the use of the lancet in the early stage of the disease to be productive of great relief. Venesection was seldom used more than once; but its effect was to shorten the duration of the disease, to lower the fever, to lessen the liability to the so-called metastases, and to render the whole case much more amenable to treatment. But I have not bled in rheumatic fever for the last

quarter of a century; for the whole character of the disease has changed. We have not had for many years the bounding pulse, the exaggerated heat and sweating, nor the same liability to acute inflammations of internal parts. The action of the heart is often feeble; and the tonic and supporting plan seems called for from an early period. Another point worthy of remark is, that cardiac and aortic murmurs of the anæmic kind have for many years been much more frequently observed, both during the attack and in the convalescence, demanding the use of iron for their removal. Observations of a similar kind apply to other acute diseases; such, for example, as erysipelas and other affections of the skin. Before 1830, we had, as an ordinary disease, the acute phlegmonoid erysipelas, attended with inflammatory reaction, vivid redness, and great swelling of parts. The practice of free leeching gave great relief; so also did that of incisions. All these characters have, to a great degree, disappeared.

It is needless to add more examples; let us rather turn to another kind of evidence. Hitherto the change of type has been recognised and determined less by anatomical observation than by the observation of symptoms, and still more by the application of the therapeutic test. Remedial measures of a certain kind were found to fail and to be hurtful, where they were formerly safe and successful; and, conversely, the use of a supporting system of tonics and the free employment of stimuli were found necessary and safe where formerly they did injury. To the all-important subject of the value of therapeutic study as a means of elucidating the laws of disease, I may presently return. But I think that I am in a position, from actual observation, to declare that morbid anatomy adds its testimony to the truth of these views.

The Pathological Society of Dublin has been now established for twenty-six years, during which time it has held weekly meetings for six months of each year. As one of the Secretaries of that Society, I have had full opportunity of seeing and examining

the recent examples of diseased structure brought weekly before the body—amounting to nearly 3,000 specimens—the collected products of the various hospitals of the city; and this result is remarkable, that the specimens of acute disease have had a character very different from that commonly met with in Dublin between 1820 and 1830. As a general rule, these specimens all showed appearances indicative of a less degree of pathologic energy. In pneumonia, for example, the redness, firmness, compactness, and defined boundary of the solidified lung was seldom seen; and that state of dryness and vivid scarlet injection, to which I ventured to give the name of the first stage of pneumonia, became very rare. In place of these characters, we had a condition more approaching to splenisation—the affected parts purple, not bright red; friable, not firm; moist, not dry; and the whole looking more like the result of diffuse than of energetic and concentrated inflammation; or we had another form, to which Dr. Corrigan has given the name of blue pneumonia, in which the structure resembled that of a carnified lung which had been steeped in venous blood.

Let us turn now to the serous membranes, and the same story is repeated. The high arterial injection, the dryness of the surface, the free production, close adhesion, and firm structure of the false membranes in acute affections of the arachnoid, pericardium, pleura, and peritoneum, with which we were so familiar before the time in question, ceased in a great measure to make their appearance. The exudations were more or less hæmorrhagic; the effused lymph lying like a pasty covering rather than a close and firm investment; it was thin, ill defined, and more or less transparent. In many of such cases, during the disease, as the late Dr. Mayne has shown in his memoir on pericarditis, friction-sounds were never presented. Serous or sero-fibrinous effusions tinged with colouring matter replaced the old results of sthenic inflammations, and all tallied exactly with the change in the vital character of the disease.

It has happened to me—and I mention this in evi-

dence that we were not mistaken as to cases peculiar to the sthenic form—that a few instances of disease in its old inflammatory characters have appeared in isolated examples, and at irregular intervals of time; so that we at once recognised their nature, and employed with success the old treatment in all its vigour—employed the lancet, although for many years its use had not been resorted to. This is very important, as showing that there are influences, the nature of which is as yet unknown, that affect the vital character of local diseases in an inconstant manner.

In an address of this kind, it is plain that this subject cannot be handled in an exhaustive fashion; it is enough that we touch upon a few of the larger subjects of inquiry. And now it will, I hope, be admitted that, with reference to the doctrine of change of type, we have brought to bear upon it the great sources of evidence as to the nature of disease.

Of these, the first is the study of vital symptoms —that study in which the older physicians so excelled, and which, from the very necessity of the case, they probably carried further than we now do, armed as we are with the many aids of physical diagnosis;

The second is the study of the characters of the anatomical changes induced by disease, and this in a comparative way, as referring to successive periods of time;

And the third is that which is derived from the results of therapeutic experiments.

Looking at the question from any one of these points of view, we come to the conclusion, that the doctrine of change of type is a true one; while, if we take all these facts, and observe how they point to the same conclusion, we must, to use again the words of Alison, accept the change of type as an ultimate fact in the history of disease.

But are we to conclude that this asthenic type of disease is always to continue? Are we to forget that in our own time we have witnessed its advent and growth? Is it not possible—nay, probable—that we or our successors may witness its disappearance, and,

coincidently, the return to an antiphlogistic medicine, regulated and tempered by the advances in diagnosis and pathology which have been meanwhile made? I have given you the opinion of Dr. Christison on this matter; let us now hear Dr. Watson:—

"I am firmly persuaded by my own observations, and by the records of medicine, that there are waves of time through which the sthenic and asthenic characters of disease prevail in succession, and that we are at present living in one of its adynamic phases."

It is very important that the change of treatment of fevers and acute local disease be traced to its true sources. This change has not proceeded from any advance in our knowledge of physiology or of pathological anatomy, nor from any new principles of practice announced as applicable to all time, and therefore implying that our predecessors were groping in the dark, or wilfully and ignorantly following a system of traditional error. To each one of us the honour of our profession, which includes its scientific character and its power of development out of itself, has been intrusted. Medicine, like other professions involving human interests, has been continually assailed from without, and harmlessly. Attacks on her honour proceeding from her own children, no matter what amount of ability may be shown, while they inflict a deeper wound, ever recoil upon their authors. This has been well exemplified in the case of Paracelsus, who burned the books of the Greek, Roman, and Arabian physicians. It is well exemplified in the case of Broussais, who, in speaking of the Eclectics, spares no term of contempt. According to him, they were guilty of shocking contradictions and absurdities, even of imposture. But, he says, "What matter for all this? Falsehood is no longer a vice. Its apotheosis has been made by this famous party, who think that they are to reign for ever." He goes on to speak of their gratuitous suppositions, of their assertions void of truth, of false imputations, inaccurate quotations, and impudent denials and total perversion of the use of words. He is the only light, and in his devotion to truth he has scorned the miserable ambi-

tion of practising in gilded saloons, and the possession of the honours of the profession.

In common with Dr. Christison, I have to express my regret, if in the discussion of this great question I have to introduce something of the controversial element. Let us inquire whether the distinguished Professor of Clinical Medicine in Edinburgh, as well as Dr. Markham, have not in some degree followed the examples of Paracelsus and of Broussais, not indeed in violence of language and indiscriminate denunciation, of which they are incapable, but in the attempt to show that their predecessors were deficient in observation and erroneous in practice.

In the very limited analysis which I have given of Professor Christison's views on the subject of the change of type, and in the statement of such opinion as I have been led to form on the question, I have at least endeavoured to show that it is not one which is to be lightly disposed of. When I had determined on the general nature of a discourse fitted for this occasion and this audience, I felt a difficulty, on recollecting that in 1861 Dr. Markham, who then filled the place that I do now, had in his address argued against the doctrine in question. But, on referring to the *Gulstonian Lectures* of 1864, I was happy to find that Dr. Markham, when questioning the value of his predecessors' observations, does them the justice of declaring his belief, that, as they have advised their followers to try all things by the light of their own reasoning and observation, they will be the last who would object to the freest criticism of their opinions. Let me, who am one of those who hold views opposite to those of Dr. Markham, gladly reciprocate the compliment. It is but justice to Dr. Markham to remind you, that he holds that general and local bleeding are remedies of great value when employed on fit occasions, and that at the present day cases are sometimes injured from our timidity in using them.

It appears pretty certain, that the change in treatment of such physicians as Alison, Christison, Watson, and Graves, did not solely spring from the

results of the therapeutic test; but that the study of the symptoms and general characters of disease was equally considered by them. In this change, too, they and their many followers in the three kingdoms have only done that for which a knowledge of the history of medicine has given abundant precedents. It would be well if it could be remembered that in the study of disease we are to look beyond anatomy, and beyond physiology—as Professor Autenrieth well observes, the "*constitutio morborum stationaria*" of Sydenham has been nearly forgotten, or else confounded with the permanent influence of the seasons, or accidental atmospheric changes.

"All diseases, contagious and non-contagious, acute and chronic (the latter, however, seldom except when attended with some degree of general excitement) have been observed to preserve a certain constitution or general character, which continues for a number of years in succession, with occasional interruptions, until it is replaced by another constitution of a different kind."

"Again," he says, "accurate observations are still wanting to determine how this periodic constitution is confined to certain parts of the world, or extends over the whole, and whether its different species follow each other in a regular order of succession. If such should at any time be determined, it will enable the physician to foretell the character and most appropriate treatment of future diseases.

"The general indications, of course, vary with the nature of the prevailing constitution; and consequently during one period stimulating remedies, during another alvine evacuations, during a third venesection and the antiphlogistic plan, will constitute the most effectual treatment."

Let me now read another passage from Autenrieth. It is not very flattering, I admit; but it is well to know what other people think of us. This was written a quarter of a century ago.

"This very circumstance has caused much confusion in medical opinions, and has occasioned the reputation and the downfall of many an infallible

system, each of which is in its turn consigned to oblivion, and perhaps again revived as a novelty at some future period. The English boast much of the astonishing improvements in science, and deride the ignorance of their predecessors, regardless of the old proverb—'Everything has its day.' Whenever, therefore, the periodic constitution undergoes an alteration, they either obstinately uphold their usual plan of treatment to the manifest injury of their patients, or else blindly embrace some system, to them new, but which really rests upon ancient and established principles. In general, they do not fail to make use of so much exaggeration in support of their opinions, and thus succeed in misleading so many, that none but very well informed physicians can distinguish the fallacy of their arguments.

"The medical history of Great Britain affords many striking proofs of the truth of these assertions, and is replete with examples of the singular obstinacy with which the English cling to opinions once formed, a circumstance which has materially contributed to obstruct their attaining to general views and impartial conclusions. Even to this day, a warm contest is carried on (less, however, in books than in the debates of learned societies) between the senior and the junior parts of the profession, the former still inclining to Brunonianism, while the latter attribute nearly all diseases to inflammation. Both, indeed, appeal to experience to prove the justice of their principles, and seem entirely to forget that, while the propriety of their practice, as applied to particular cases, remains unimpeached, the very nature of the diseases themselves may have been changed."

There is a statement made by Dr. Bennett in his great work on the *Practice of Medicine*, which is at least a startling one. It is, that in his treatment the mortality of pneumonia has been reduced by a large percentage. The mortality in Edinburgh, according to him, under the system of his predecessors, was no less than one in three—equal to that in the first outbursts of Asiatic cholera. The statistics belong to two periods—namely, from 1839 to 1849,

and from 1812 to 1837. The mortality in the first division was even more than one in three. The results of other statistics are also given, of which the most valuable are those of Louis, who brings out the mortality of those bled moderately, and at an early period, as 1 in $7\frac{1}{4}$, and of those in which the blood was taken at any time from the first to the ninth day, as 1 in $3\frac{1}{2}$.

Now, it is unnecessary to remind an audience constituted as this one is, of the difficulties which attend medical statistics in general, and those relating to therapeutics in particular. When I began the study of medicine, pneumonia was considered to be far more manageable than other acute visceral inflammations; and that its rapid retrocession took place under the old treatment in a vast number of cases, no man can doubt who lived and practised in that time. But in these statistics of pneumonia I find an omission. If we look at the diagnosis of this disease in a purely physical point of view, we run the risk of committing the great error of confounding cases, the constitutional nature of which is widely different—cases on the one hand of original idiopathic pneumonia occurring from accidental causes, and cases in which the change in the lung is secondary to some form of fever. And this makes a most important difference. It is my conviction that many of the so-called cases of pneumonia which have occurred in the United Kingdom since 1830 were really examples of the latter form. But, further, it is certain that in many instances the occurrence of the pneumonia is attended with such a change in the constitutional symptoms as to deceive the practitioner, and hide from him the fact that he had to deal with a secondary, in place of a primary affection. In some cases we see a change from the essential to the symptomatic character, while in others this remarkable circumstance occurs that, coincidently with, or very soon after the development of the symptoms and the physical signs of pneumonia, the fever ceases; so that we have long come to the conclusion in the Meath Hospital, that many of these cases with every local symptom and

sign, are in truth only examples of aborted fevers, ending critically in pneumonia, just as we see, in some cases of variola, the fever ceasing with the pustulation of the skin. If these things be true, how important is their recognition—how inconsequential the conclusions as to treatment, based on statistics from which such facts are excluded!

Let us now inquire to what sources Professor Bennett traces the changes in practice, and that success in the treatment which has reduced the mortality of pneumonia from one in three to one in thirty-six in complicated cases, while in the uncomplicated cases there was no mortality at all. They are stated to be the improvements in diagnosis on the one hand, and the adoption of a practice founded on the cellular pathology on the other. As to the first, how the improvement in the diagnosis of pneumonia could have led to the change of treatment of fevers, and cerebral or abdominal inflammation, is hard to understand—this has been well put by Dr. Christison—unless it could be shown that the failure of bleeding in pneumonia led men to think it would also be useless or injurious in other diseases. It must be remembered, however, that the change in treatment began first as to fevers; and it was the observation of the change of type in that class of diseases that led to the idea, and afterwards the demonstration of a similar change as to local affections.

But improvement in the physical diagnosis of pneumonia can hardly be said to have advanced since the time of Laennec. And it is clear that, looking at therapeutics, the influence on them of any such improvement is indirect, rather than direct. We know better the seat, the period of commencement, the periods of pathologic changes—their amount in some cases, and the complications with other forms and centres of diseased action; but we get little, if any, new light as to the proper remedy. Take any or all of the three great cases of intrathoracic inflammation. The physical signs as to character and succession are essentially the same in the asthenic or typhoid forms, as in those with the highest inflammatory reaction.

Therefore, to say that the alleged improvement in treatment proceeded from the advance of the physical diagnosis, is a proposition which must be rejected.

The adoption of practice founded on the cellular pathology of Virchow is a much wider question. It is to be remarked here, that the therapeutic test of the value of these means is still to be applied. We find a treatment indicated as based on, or as directly flowing from, the new pathology. Yet what is this treatment? It consists in the use of certain measures, and the abstaining from their opposites; but neither in principle nor detail does it differ from that adopted by the older clinical physicians in asthenic local diseases, from the time of John Peter Frank downwards to that of Bateman in 1809, who was compelled to use venesection—a practice opposed to his former views. It is, then, no new treatment; and it, at all events originally, could not have been based on a pathology of which nothing was known at the time when it was first adopted. I do not say that, if the cellular pathology be finally demonstrated to be true, it will not—to adopt the expression of Dr. Bennett—be attended by cell-therapeutics as its necessary complement. New modifications of treatment may be discovered, and probably will be discovered; while on the other hand the old methods stamped by experience, and the discoveries of enlightened observation, may be brought to bear in confirming the new truths.

But, in the present state of our knowledge, it cannot yet be said that the views of Virchow have had any direct influence on the healing art—that is to say, they have not led us to any new remedy; they have not explained the action of many of the old ones.

I am not, you will believe me, speaking in a spirit of depreciation of the labours of Professor Bennett, and more especially those of Virchow. Looking at the practical results of Virchow's labours, we may separate the consideration of the cell-pathology from that which belongs to questions of another kind.

The demonstration of the whole of embolic diseases, and the investigations of both Bennett and Virchow, as to the part played by the colourless globules of the blood, are priceless additions to medicine; not, however, as giving us directly any new means of cure, but as enabling us to avoid errors into which our ignorance of these things led us before. We shall not now confound a case of embolus of the pulmonary artery with asthma or with hydrothorax; nor, in a case of amaurosis from obstruction of the central artery of the retina, shall we be led to treat it as a symptom of disease of the brain. It is plain that, whatever be the result as to practical medicine of these labours, they are to be held in the highest estimation, as evidencing the onward march of discovery in a certain direction—as enlightened efforts to place pathology on a more definite basis. Yet, I repeat it, the cellular pathology with reference to practieal medicine stands in a position analogous to that of our vastly improved diagnosis. Supposing all that is announced by Virchow, Weber, and Bennett, to be established, still up to the present time it would give to the healing art only indirect assistance.

Let us permit the elements of tissues, the ultimate cell, or granule, to share with the great compound organs of the body the property of specific action; let us admit that there is no spontaneous generation of cells from an amorphous blastema, but that every cell proceeds from a cell; that cells are the ultimate elements of animal as of vegetable structure; that differences of function depend on differences of their contents; that every animal is a sum of vital unities, every one of which manifests all the characters of life, deriving, it may be, its stimulus and intensity from other sources, but itself alone performing its actual special duties.

Let us hold, further, the whole doctrine of neoplasms, which sets forth that every pathological structure has its physiological prototype; and that even cancer has not its specific difference, any more than pus, but that its supposed peculiarity is traceable to

the stage at which we examine it; that the law of histological substitution be accepted physiologically, as when one tissue of a similar type replaces another; and pathologically, when a different tissue, but still one having its physiological prototype, comes into play. Again, if it be found to be true that all dyscrasiæ have a local origin, and are dependent on a permanent supply from a local origin; that fibrine is not a constituent of living blood; that between the pus-cell and the colourless globules of the blood there is no difference, so that the term pyæmia must be given up as a condition susceptible of morphological demonstration, but implying a complex mass of conditions, the central point of which is not a purulent infection of the blood; that embolism is the key to the study of metastasis; and that inflammation is nothing, as Bennett teaches, but an alienation of nutrition,—if, I say, all these things be true, it does not appear that they furnish knowledge that would tell us why this or that line of treatment is from time to time found efficacious—a knowledge that would direct us in the cure, though to a certain degree it might to the prevention, of disease. In truth, such knowledge throws little light on the action of medicines, on the laws of periodicity, and on the great phenomena of essential diseases, as to their origin and specific character, spread, secondary lesions, their crises, and the influence of treatment on them. And therefore I conclude that, even if, under the treatment of a distinguished professor of this science, the mortality of a frequently recurring disease has been annihilated, that result cannot be as yet traced either to a new diagnosis or a new pathology, as exercised by him; nor, conversely, can the great alleged mortality of former times be attributed to imperfect diagnosis on the one hand, or to traditional errors in treatment on the other.

But let us ask, if it be true that the mortality of pneumonia has been in the latter years so much lessened, to what is this to be attributed? It may be that we have to deal with a disease of less organic activity or tension, so that in certain cases it may,

like a fever, subside spontaneously ; perhaps, too, like a fever, under its own law of periodicity. And it is probable, at all events, that the more a local disease corresponds in its vital character to the epidemic constitution, to use the words of Sydenham, the more will it appear under the laws of periodicity, for good or for evil.

It is very hard to predicate the limits to which the study of healthy and of diseased structure in its mechanical or chemical relations may lead us ; and it is possible that even such investigations of the laws of organisation may result in giving us power to infer the existence of phenomena not yet discovered —to proceed, as Adams and Le Verrier have so gloriously done, from the seen to the unseen, and approach one degree nearer to the solution of the great problem of life in health or in disease—or, again, that of the action of medicines. But we must take heed not to leave any path of observation unexplored, nor to despise those investigators who, from necessity or choice, follow a less mechanical method, but who have made medicine progressive—every day less an art, and more a science. Every established microscopic observation in normal or in morbid anatomy—every faithful analysis of any solid or fluid in the body—every discovery as to the spectral phenomena of the blood or of the liquid secretions of any gland, nay, of any cell in the organism, though to a one-sided view it appears useless, is truly a precious thing.

And here it is fitting to remark, that there is nothing in the doctrine of cellular unity, of cellular independent action, and in all the processes of cell-growth, proliferation, and decay, which is inconsistent with the doctrine of change of type of disease. Dr. Bennett allows that change of type may be admitted as to essential diseases, but seems to hold that, as to acute or chronic organic disease, the doctrine is to be rejected. But, if there be a change of type in the essential diseases, it is difficult to understand why there should not be a change in character of the secondary effects of those diseases, whether these are met with in the nervous centres, in the thoracic or

abdominal organs; so that we may have sthenic or asthenic cell-growths, cell-proliferations, cell-transmutations, and cell-decay; and all this without referring to the notion that, because local disease, as well as general, exhibits now a lower activity, therefore the physical state of man *in health* has deteriorated.

We are still very far from determining the laws of the so-called zymotic diseases; but this seems certain, that at the invasion of epidemics the strong man is struck down, and often exhibits the phenomena of the disease in the most aggravated forms. In the epidemic of 1827 in Ireland, nothing was more remarkable than this, that its virulent forms, especially that in which it so closely resembled the yellow fever of the tropics, were at first seen in the finest and strongest men. It is even probable that in these diseases the very vigour of the system may imply a greater malignity or activity of the processes which constitute the disease. The existence, then, of a changed type of disease may be admitted without the necessity for believing that the human species has degenerated.

Before concluding, it is right that we should consider the relations of therapeutics to medicine. It will be admitted by most thinking men that the study of diseased or healthy organisation has revealed more of the effects than of the essence of disease. So subtle are the conditions by which the quality of life is preserved, that, in a vast proportion of instances of death, the most refined anatomy and chemistry fail in discovering a commensurate change, or in explaining why what was a living creature yesterday lies before us in a few hours a decomposing mass of clay. Hence, we must be cautious in extensively adopting any therapeutical system which is solely based on inference from visible organic change. In the present imperfect state of our knowledge, we must not neglect that study of therapeutics which is essentially experimental and inductive; and if there be one thing wanting more than another in our science, it is that men should know the nature and difficulties of thera-

peutic evidence. If, as I have often heard Professor Acland observe, only a few of our well instructed brethren who are in charge of public institutions, well aware of the established laws of disease, whether essential or non-essential, and good observers, were to take up any one remedy, whether new or old, say digitalis, and faithfully record on the one hand the character and history of the case, and on the other the results of the use of the particular medicine, or other therapeutical proceeding, we should ere long have such a mass of unbiassed statement of facts, that safe conclusions could be drawn. Until this is done, the position of therapeutics will be an inferior one. It will not be any trustworthy guide in practice, except in a few salient instances, and will be powerless in its other great function of being the key to, and the test of, pathologic conclusions.

To bring therapeutics up to this level, seems to be the great desideratum. We may fairly hold that the time is ripe for the commencement of its study with the view to its higher functions or development. Without placing limits to the material investigations in which we are aided by the microscope and by chemistry, we may believe that our knowledge of the intimate structure and composition of the solids and fluids of the body is so extended, as to give to the therapeutist reason for holding that he is now far better acquainted with the living organism than he was a quarter of a century ago ; and that so he has a broader and more secure foundation to build upon. But the therapeutist must also possess assistance of another kind. He must know the principles of accurate reasoning ; he must distinguish between the *post hoc* and the *propter hoc;* he must be content still to deal with vital phenomena as constituting a class of the nature of which our knowledge is so deficient, that we have still to study their modifications by external agents, experimentally, and without as yet much reference to their relations to structure or to vital chemistry ; he must take into account the laws of periodic action in health and in disease, and determine, or seek to

determine, as he proceeds, whether the simplest form of acute local as well as of general disease is not under some of these wonderful laws; he must study the question as to whether medicinal interference extinguishes morbid action, postpones it, or, by breaking its circle, as suggested by Professor Boeck, though this be followed by temporary good, deranges the process which is to end in its removal; he must well understand that certainty in medicine must be approached by the balance of probabilities, and have a full insight into the difficulties of medical statistics, which result from the labours of more than one observer. Other circumstances will suggest themselves to you—as the influences of locality, of race, of age, sex, habit, and previous history. I will not dwell on them, further than to remark that, had Broussais attended to one of them, in particular, he would not, I think, have fallen into the error of declaring the non-existence of essential fever from observing disease within a narrow circle of the world.

If therapeutic science is to advance, it must be followed and studied in the most severe scientific spirit.

I have to thank you for the courtesy and patience with which you have listened to this somewhat dry discourse. We have indeed dealt with important subjects; and you will, I hope, believe me when I say, that no one in this room can feel more than I do how defective has been their handling. I say this in no guise of mock modesty. But I have endeavoured to speak as a practical physician, who has worked for forty years, to a body of his brethren engaged like him in fighting the same battle, using the same weapons, and bringing all their powers to insure the same result.

NOTE.

There are some additional points to which I had intended to allude in the foregoing address; but which were omitted from the fear of making the discourse too tedious. Of these, the most important